Supplemental Materials: What politicians don't know can hurt you: The effects of information on politicians' spending decisions.

Ryan S. Jablonski[1] Brigitte Seim[2]

[1]London School of Economics and Political Science
[2]University of North Carolina, Chapel Hill

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Note *this is an abbreviated version of the supplemental materials (pages 1-25). A complete version of the supplementary materials (pages 1-70), including several analyses referenced in the main text can be found on the online dataverse at* `https://doi.org/10.7910/DVN/HS5R5S`*.*

Contents

1 Overview

These abbreviated Supplemental Materials (SM) are intended to provide additional information useful for understanding the experiment and the results in the main text. In addition to the material here, you can find on the associated dataverse complete supplementary materials, an example survey, replication files, extended versions of some results tables, and ethics approvals.

1. Section 2 provides an illustrative model of Bayesian updating. We derive the conditions under which information improves distributional decisions and under which we can expect positive treatment effects.

2. Section 3 provides tables for all the estimates plotted in the main text.

3. Section 4 provides additional tests that might aid in understanding the results of the study, including a power analysis, multiple comparison tests, assessments of experimenter demand effects, compliance checks and interactions across treatment arms. We also consider alternative ways politicians may use voting information in their spending strategies.

In the extended materials (included on the dataverse) you can find these additional analyses:

1. In Section 5, we discuss evidence of learning and updating. We discuss post-treatment surveys that indicate that politicians retained information and found it useful. We also show tests of conditional treatment effects by politician knowledge of their constituencies.

2. Section 6 provides statistics on sample representativeness, attrition, variable correlations, and variable distributions and coding details.

3. Section 7 provides an overview of the pre-treatment interview protocol and a description of the survey of citizens and teachers referenced in the main text.

4. Section 8 provides a detailed description of the randomization process, example maps, details on the goods used in the experiment, and example transparency treatments.

5. Section 9 provides a discussion of the ethics of this experiment and the steps we took to ensure the protection of all research participants.

6. The full SM also serves as a compendium of all the tests of the information treatment arms which were pre-specified in our pre-analysis plan (PAP). This pre-analysis plan was filed with EGAP on January 23, 2018 prior to any analysis being undertaken. You can see the full pre-analysis plan at `https://osf.io/kazfp`. Additionally, in Section 10, we summarize all of the pre-specified hypothesis tests and where the tests can be found. Finally, we discuss deviations from the PAP in Section 11.

2 Formal Model of Information Updating

In this section, we formally derive the assumptions required for our hypotheses about information updating to hold.

In line with our experimental setup, consider a politician that has to make a decision about how to allocate a fixed development budget of value $a > 0$ to a set of schools n. In making this decision, the politician has to consider the returns (e.g., in terms of votes or welfare) to each investment, $v_i ... v_n$. We assume $v_i(a) > 0 \quad \forall i$. We define $v_i > v_{i+1}$; which implies that a completely informed politician will always prefer to spend on school one.

We represent the politician's prior beliefs about each v_i as independent random variables $\phi_i ... \phi_n$. We assume that these priors are normally distributed with means $m_i ... m_n$ variances $\sigma_i^2 ... \sigma_n^2$. To simplify the exposition, we will assume for now that $n = 2$ and that prior variance is constant ($\sigma_i^2 = \sigma_{i+1}^2$). Later we discuss the implications of relaxing these assumptions.

Let θ equal the probability that $\phi_1 > \phi_2$. θ is therefore equal to the probability that the politician obtains maximum returns to her investment, which can be represented as follows:

$$\theta = Pr(\phi_1 - \phi_2 > 0) = \Phi_0(m_2 - m_1, \sigma_1^2 + \sigma_2^2) \tag{S1}$$

Where Φ_0 is the normal CDF evaluated at 0.

From Equation S1, it follows that there is a positive relationship between information precision and accuracy and the probability of making an effective spending decision. From the properties of the normal CDF, it follows that the probability of inefficient distributional decisions (θ) is increasing in prior inaccuracy ($m_2 - m_1$). Further when priors are inaccurate ($m_2 > m_1$), the probability of an inefficient decision is increasing in total uncertainty ($\sigma_1^2 + \sigma_2^2$).

This simple model likely also underestimate the effects of uncertainty. Many models assume a negative correlation between v_j and σ_j^2. For instance, if politicians require some knowledge about a community in order to optimize the way spending is delivered, or in order to claim political credit for that spending, then politician utility is likely going to be higher where politicians have better knowledge (for discussion see, e.g., Keefer and Vlaicu 2008; Stokes 2007; Dixit and Londregan 1996). In the context of our experiment, for instance, politicians often showed up during deliveries of school goods to engage in claim some credit for the aid. Such credit claiming activities are likely only possible where politicians have close personal connections with a community.

A negative correlation between v_j and σ_j^2 implies that communities with higher information costs will be especially disadvantaged in spending decisions.

In line with our experiment, we assume that some politicians receive a treatment signal about the correct values of v_1 and v_2. We represent these signals as random variables τ_1 and τ_2. We assume these signals are normally distributed with truthful means equal to v_1 and v_2 and constant variance s^2.

If the politician updates using Bayes' rule, their posterior beliefs in the treatment condition are as follows:

$$\phi_i(\tau_i) = N[m_i + (v_i - m_i)\lambda_i, \frac{\sigma_i^2 s^2}{\sigma_i^2 + s^2}] \tag{S2}$$

Where $\lambda_i = \frac{\sigma_i^2}{\sigma_i^2 + s^2}$ equals the precision of the information signal. A politician's decision problem is to determine the probability that $\phi_1(\tau_1) > \phi_2(\tau_2) = \theta(\tau_1, \tau 2)$, which is equal to the cumulative distribution function of $\phi_2(\tau_2) - \phi_1(\tau_1)$ evaluated at zero:

$$\theta(\tau_1, \tau_2) = Pr[\phi_2(\tau_2) - \phi_1(\tau 1) > 0] = \Phi_0\{[m_2 + (v_2 - m_2)\lambda_2] - (m_1 + (v_1 - m_1)\lambda_1], \frac{\sigma_2^2 s^2}{\sigma_2^2 + s^2} + \frac{\sigma_1^2 s^2}{\sigma_1^2 + s^2}\} \tag{S3}$$

We can now derive the conditions under which information improves spending decisions ($\theta(\tau_1, \tau 2) > \theta$), and, by implication, those conditions under which treatment effects will be positive. Under reasonable assumptions, we can show that politicians will never be worse off with information than without information, and will most often be better off. Unlike in updating models with one-sided information, this conclusion does not depend upon the accuracy or ranking of a politician's priors, m_1 and m_2.

To illustrate why this is, first consider the case where a politician correctly ranks schools ($m1 > m2$). If information causes incorrect decisions, it would have to be the case that information causes a politician to switch to the school with lower returns, which would occur only if the posterior means implies higher returns for school two than school one. From Equation S3 this would imply the following would have to be true:

$$m_1 + (v_1 - m_1)\lambda_1 < m_2 + (v_2 - m_2)\lambda_2 \tag{S4}$$

Since our assumption that $v_1 > v_2$ and $m_1 > m_2$ would contradict equation S4, it follows that this can never be the case.[1]

Proposition 1 *When a politician has correct priors ($m_2 > m_1$) with consistent variance ($\lambda_1 = \lambda_2$), the probability of correctly ranking the schools will never be lower in the treatment condition than the control condition ($\theta(\tau_1, \tau 2) \not< \theta$).*

[1] After simplifying, $m_1 - m_2 + \lambda v_1 - \lambda v_2 < \lambda m_1 - \lambda m_2$. Since λ is positive and bounded between zero and one, this can never be the case.

Now consider the case where a politician has an incorrect ranking ($m_2 > m_1$). If information causes worse decisions, it would have to be the case that the informative signal makes it *more* likely that a politician retains rather than switches their ranking. From Equation S3, this would imply

$$Pr[m_1 + (v_1 - m_1)\lambda_1 < m_2 + (v_2 - m_2)\lambda_2] > Pr(m_1 < m2) \tag{S5}$$

Since, by assumption, $v_1 - m_1 > v_2 - m_2$, the probability of switching to a more accurate ranking of schools is always higher in the treatment condition and this can never be the case.[2]

Proposition 2 *When a politician has incorrect priors ($m_2 > m_1$) with consistent variance ($\lambda_1 = \lambda_2$), the probability of a correct school ranking is higher in the treatment condition than the control condition ($\theta(\tau_1, \tau 2) > \theta$).*

It follows similarly that the probability that a politician switches to a more effective spending decision is greater when priors are more diffuse or when the information signal is more precise. To illustrate, note that when a politician has an incorrect ranking ($m_2 > m_1$), the probability she changes her ranking is equal to $Pr[m_1 + (v_1 - m_1)\lambda > m_2 + (v_2 - m_2)\lambda]$. Again, since $v_1 - m_1 > v_2 - m_2$, it follows that this probability is strictly increasing in λ. For this reason, we predicted in our experiment that politicians would be more responsive to information treatments when the precision of information priors are limited by high information costs (e.g., due to the costs of travel to distant schools).

Proposition 3 *When a politician has incorrect priors ($m_2 > m_1$) with consistent variance ($\lambda_1 = \lambda_2$), the probability of a correct ranking is increasing in the precision of the signal ($1/s^2$) and the variance in prior beliefs (σ_i^2).*

The conclusions above assume that the politician's priors are similarly precise for both schools ($\lambda_1 = \lambda_2$). We might doubt this is the case. As we discuss in the main manuscript, politicians are better informed about some communities than others, for instance due to the greater costs of citizen lobbying in more distant communities. Additionally, motivated reasoning or partisan bias might also motivate differences in the precision of priors (Gerber and Green, 1999).

It follows directly from Equation S3 that the change in a politician's posterior beliefs is proportional to λ_i and m_i. Therefore a primary effect of relaxing this assumption is to vary the probability that a politician updates their beliefs about a particular school. Formally, it follows from Equation S3 that:

Proposition 4 *When a politician has less precise priors about school i than school $i+1$ ($\sigma_i^2 < \sigma_{i+1}^2$), the difference between priors and posteriors will likewise be greater for school i than school $i+1$. Formally, $(m_i + (v_i - m_i)\lambda_i) - [m_{i+1} + (v_{i+1} - m_{i+1})]\lambda_{i+1} > (m_i - m_{i+1})$.*

Another implication of relaxing the constant variance assumption is that Propositions 1 and 2 will not hold under all conditions. To see this, note that equations S4 and S5 are not contradicted with certainty if we do not restrict the distribution of λ_1 and λ_2. When $\lambda_1 \neq \lambda_2$, politicians might become less likely to select school one in the treatment condition ($\theta(\tau_1, \tau_2) < \theta$). To illustrate, suppose a politician received information that causes her to update negatively ($m_1 > v_1$ and $m_2 > v_2$) but at different rates ($\sigma_1^2 > \sigma_2^2$). In such a scenario, if differential updating is considerably greater for school one than school two, then it is conceivable that a politician will switch from preferring school one to preferring school two in the treatment condition.

We refer to this scenario as negative updating. Negative updating will occur especially when the precision of priors are much greater for the second school ($\sigma_1^2 > \sigma_2^2$) and when politicians are close to indifferent in their priors ($m_1 \sim m_2$). The range of values where this occurs are narrow and occur rarely across reasonable simulations.[3]

There are also empirical reasons to discount negative updating. Negative updating requires that politicians know *less* about schools with a higher investment return. This contradicts what we show in the main manuscript (e.g., politicians know more about high need schools). Additionally, we think it unlikely that politicians who are relatively indifferent in their priors will have large differences in the precision of their beliefs. Most evidence and theory suggests instead that indifference is correlated with less confident beliefs (Feddersen and Pesendorfer, 1996; Druckman and Lupia, 2000).

We next relax the assumption that politicians are only selecting between two schools. While the logic becomes more complex, our core conclusion about the beneficial effects of information do not change. Assume, for instance, the setting in our experiment of a politician allocating across three schools with returns v_1, v_2, and v_3. Consider, first, the case of consistent priors ($m_1 > m_2 > m_3$). Here m_3 is irrelevant to the decision and the logic simplifies to the comparison between v_1 and v_2. Alternatively, a politician might have incorrect priors ($m_3 > m_2 > m_1$) or ($m_3 > m_1 > m2$). In order for $\theta > \theta(\tau_1, \tau_2, \tau_3)$, one of the following must hold:

$$Pr(m_1 + (v_1 - m_1)\lambda_1 < m_2 + (v_2 - m_2)\lambda_2) > Pr(m_1 < m2) \tag{S6}$$

$$Pr(m_1 + (v_1 - m_1)\lambda_1 < m_3 + (v_3 - m_3)\lambda_3) > Pr(m_1 < m3) \tag{S7}$$

We already ruled out the first possibility (Equation S5) and the second inequality is impossible for an identical reason: By assumption, $v_3 - m_1 > v_3 - m_1$ so this inequality cannot hold. We could make a similar argument for any set of n schools.

[2] After simplifying, $v_1 - v_2 > m_1 - m_2$. By assumption, $v_1 - v_2$ is strictly positive and $m_1 - m_2$ is strictly negative.

[3] In simulations assuming independent and uniform distributions on m_i and v_i bounded between 0 and 1, we observed negative updating less than 2% of the time and positive updating over 70% of the time.

It's important to note that this model of information updating is different in important ways from one-sided information problems that we see, e.g., in theories of voting behavior. In models and experiments of how information affects voting, voters generally only receive information about the quality of incumbents (e.g., the level of incumbent corruption). How voters respond to that information depends upon whether the information causes voters' relative ranking of incumbent and challenger to shift positively or negatively. Because the direction of this shift depends in turn on politician priors, the average effects of information, independent of priors, can be indeterminate (for discussion see Arias et al. 2018 and Izzo, Dewan and Wolton 2018). In contrast, in our setting, politicians receive information about the full set of possible schools. In this setting, and under the assumption discussed above, a politician's ranking about the optimal investment cannot shift in a direction adverse to the ranking provided in the experiment.

3 Tables Showing Estimates from Main Text Figures

In the main manuscript, we show most treatment effect estimates in coefficient plots. In this section, we show estimates in Table form for all these plots.

3.1 Figure 2

Table S1: Estimates from Main Text Figure 2

	Linear Effect	0-25 perc.	25-50 perc.	50-75 perc.	75-100 perc.
	(1)	(2)	(3)	(4)	(5)
Log Distance from Hometown	-0.095^{***}				
	(0.022)				
Intercept	0.513^{***}	0.414^{***}	0.272^{***}	0.250^{***}	0.212^{***}
	(0.058)	(0.039)	(0.042)	(0.037)	(0.029)
Observations	1,864	495	453	511	405
R^2	0.027	-0.000	0.000	0.000	0.000

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

3.2 Figure 7

Table S2: Estimates from Figure 7 (School Need Index)

	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)
School Need Index	0.066	0.107	0.091	0.011
	(0.049)	(0.063)	(0.059)	(0.087)
Observations	1,743	1,743	1,197	546
Pseudo-R^2	0.001	0.021	0.002	0.00003

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 73).

Table S3: Estimates from Figure 7 (School Need Index*Distance)

	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)
School Need Index*Log Distance from Hometown	-0.095	-0.095	-0.133^{**}	0.057
	(0.064)	(0.064)	(0.072)	(0.146)
School Need Index	0.094	0.094	0.116^{*}	0.034
	(0.059)	(0.059)	(0.069)	(0.118)
Log Distance from Hometown	-0.092	-0.092	-0.053	-0.179
	(0.069)	(0.069)	(0.081)	(0.133)
Observations	1,287	1,287	926	361
Pseudo-R^2	0.005	0.005	0.007	0.006

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 75).

Table S4: Estimates from Figure 7 (Incumbent Votes)

	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)
Incumbent Percent	0.162***	0.162**	0.201**	0.104
	(0.065)	(0.073)	(0.084)	(0.103)
Observations	1,683	1,683	1,161	522
Pseudo-R^2	0.004	0.020	0.005	0.002

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 77).

Table S5: Estimates from Figure 7 (Family Attends School)

	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)
Family Attends School	0.537***	0.428***	0.550***	0.458
	(0.144)	(0.149)	(0.156)	(0.384)
Observations	3,492	3,492	2,439	1,053
Pseudo-R^2	0.004	0.019	0.005	0.001

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 79).

Table S6: Estimates from Figure 7 (Aid Project Count)

	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)
Aid Project Count	0.118	−0.215	0.121	0.110
	(0.079)	(0.164)	(0.094)	(0.147)
Observations	1,752	1,752	1,218	534
Pseudo-R^2	0.001	0.025	0.001	0.001

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 81).

Table S7: Estimates from Figure 7 (Population Density)

	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)
Pop Density at School	−0.030	0.105	−0.006	−0.095
	(0.049)	(0.296)	(0.059)	(0.120)
Observations	3,375	3,375	2,427	948
Pseudo-R^2	0.0001	0.021	0.00000	0.001

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 83).

3.3 Figure 8

Table S8: Estimates from Main Text Figure 8 (part 1)

	All Surveys (1)	with Controls (2)	Councillors (3)	MPs (4)
Need Treatment* School Need Index	0.074^{**}	0.082^{**}	0.089^{**}	0.031
	(0.038)	(0.039)	(0.046)	(0.068)
School Need Index	0.036	0.061^{*}	0.050	0.006
	(0.027)	(0.031)	(0.033)	(0.047)
Need Treatment				
	(0.000)	(0.000)	(0.000)	(0.000)
Aid Good Types		0.364		
		(0.232)		
Aid Project Count		−0.428		
		(0.314)		
Family Attends School		0.430^{***}		
		(0.149)		
Incumbent Percent		0.710^{***}		
		(0.234)		
Log Enrollment		0.122^{***}		
		(0.044)		
Log Permanent Classrooms		−0.075		
		(0.118)		
Log Permanent Houses		0.023		
		(0.062)		
Log Teachers		0.041		
		(0.101)		
Log Temporary Classrooms		−0.091		
		(0.070)		
Log Temporary Houses		0.029		
		(0.063)		
Log Turnout		-0.208^{**}		
		(0.084)		
Opposition Percent (LC)		−0.207		
		(0.273)		
Percent Votes (MP)		0.196		
		(0.240)		
Pop Density at School		−0.003		
		(0.003)		
Observations	3,492	3,492	2,439	1,053
Pseudo-R^2	0.005	0.020	0.009	0.001

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S9: Estimates from Main Text Figure 8 (part 2)

	Transparency Interactions
Need Treatment*School Need Index*Transparency Treatment	0.065
	(0.089)
Need Treatment*School Need Index	0.024
	(0.077)
School Need Index*Transparency Treatment	−0.001
	(0.063)
Need Treatment*Transparency Treatment	
	(0.000)
School Need Index	0.037
	(0.055)
Need Treatment	
	(0.000)
Transparency Treatment	
	(0.000)
Observations	3,492
Pseudo-R^2	0.006

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

3.4 Figure 9

Table S10: Estimates from Main Text Figure 9 (part 1)

	All Surveys	with Controls	Alternate Coding	Councillors	MPs
	(1)	(2)	(3)	(4)	(5)
Aid Treatment*Aid Project Count	-0.203^{*}	-0.220^{**}		-0.372^{***}	0.164
	(0.113)	(0.115)		(0.136)	(0.206)
Aid Project Count	0.118	0.073		0.121	0.110
	(0.079)	(0.083)		(0.094)	(0.147)
Aid Treatment*Aid Good Types			-0.227^{*}		
			(0.120)		
Aid Good Types			0.206^{***}		
			(0.086)		
Aid Treatment					
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Aid Project Count		0.424^{***}			
		(0.149)			
Family Attends School		0.723^{***}			
		(0.234)			
Incumbent Percent		0.118^{***}			
		(0.044)			
Log Enrollment		-0.060			
		(0.118)			
Log Permanent Classrooms		0.031			
		(0.062)			
Log Permanent Houses		0.063			
		(0.101)			
Log Teachers		-0.086			
		(0.070)			
Log Temporary Classrooms		0.029			
		(0.063)			
Log Temporary Houses		-0.227^{**}			
		(0.084)			
Log Turnout		-0.175			
		(0.273)			
Opposition Percent (LC)		0.201			
		(0.240)			
Percent Votes (MP)		-0.003			
		(0.003)			
Pop Density at School		0.104^{***}			
		(0.024)			
Observations	3,492	3,492	3,492	2,439	1,053
Pseudo-R^2	0.001	0.019	0.002	0.003	0.004

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S11: Estimates from Main Text Figure 9 (part 2)

	Transparency Interactions
Aid Treatment*Aid Project Count*Transparency Treatment	−0.359
	(0.265)
Aid Treatment*Aid Project Count	0.072
	(0.231)
Aid Project Count*Transparency Treatment	0.141
	(0.177)
Aid Treatment*Transparency Treatment	
	(0.000)
Aid Project Count	0.017
	(0.150)
Aid Treatment	
	(0.000)
Transparency Treatment	
	(0.000)
Observations	3,492
Pseudo-R^2	0.001

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

3.5 Figure 10

Table S12: Estimates from Main Text Figure 10 (part 1)

	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)
Voting Treatment*Incumbent Percent	0.019	0.022	−0.040	0.116
	(0.090)	(0.091)	(0.115)	(0.149)
Incumbent Percent	0.162***	0.143**	0.201**	0.104
	(0.065)	(0.069)	(0.084)	(0.103)
Voting Treatment				
	(0.000)	(0.000)	(0.000)	(0.000)
Aid Good Types		0.357		
		(0.232)		
Aid Project Count		−0.421		
		(0.313)		
Family Attends School		0.427***		
		(0.149)		
Log Enrollment		0.125***		
		(0.044)		
Log Permanent Classrooms		−0.063		
		(0.118)		
Log Permanent Houses		0.024		
		(0.062)		
Log Teachers		0.032		
		(0.102)		
Log Temporary Classrooms		−0.099		
		(0.070)		
Log Temporary Houses		0.026		
		(0.063)		
Log Turnout		−0.242***		
		(0.088)		
Opposition Percent (LC)		−0.181		
		(0.273)		
Percent Votes (MP)		0.198		
		(0.240)		
Pop Density at School		−0.003		
		(0.003)		
School Need Index		0.106***		
		(0.024)		
Observations	3,482	3,482	2,429	1,053
Pseudo-R^2	0.004	0.019	0.004	0.005

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S13: Estimates from Main Text Figure 10 (part 2)

	Transparency Interactions
Voting Treatment*Incumbent Percent*Transparency Treatment	−0.149
	(0.211)
Voting Treatment*Incumbent Percent	0.132
	(0.184)
Incumbent Percent*Transparency Treatment	0.065
	(0.153)
Voting Treatment*Transparency Treatment	
	(0.000)
Incumbent Percent	0.112
	(0.134)
Voting Treatment	
	(0.000)
Transparency Treatment	
	(0.000)
Observations	3,482
Pseudo-R^2	0.004

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

3.6 Figure 11

Table S14: Estimates from Main Text Figure 11 (Need Interactions)

	Distance Interactions (1)	Density Interactions (2)	Voting Interactions (3)
Need Treatment*Log Distance from Hometown*School Need Index	0.048		
	(0.047)		
Need Treatment*Incumbent Percent*School Need Index			0.022
			(0.039)
Need Treatment*Pop Density at School*School Need Index		−0.104*	
		(0.065)	
Need Treatment*School Need Index	0.057	0.073*	0.073*
	(0.045)	(0.039)	(0.038)
Need Treatment*Log Distance from Hometown	−0.070		
	(0.097)		
Need Treatment*Pop Density at School		−0.084	
		(0.139)	
Need Treatment*Incumbent Percent			−0.109
			(0.091)
Log Distance from Hometown*School Need Index	−0.052		
	(0.035)		
Pop Density at School*School Need Index		0.159***	
		(0.054)	
Incumbent Percent*School Need Index			−0.019
			(0.028)
Need Treatment			
	(0.000)	(0.000)	(0.000)
School Need Index	0.052	0.044	0.038
	(0.033)	(0.028)	(0.027)
Incumbent Percent			0.230***
			(0.065)
Log Distance from Hometown	−0.093		
	(0.069)		
Pop Density at School		−0.075	
		(0.096)	
Observations	2,612	3,375	3,482
Pseudo-R^2	0.009	0.011	0.010

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S15: Estimates from Main Text Figure 11 (Aid Interactions)

	Distance Interactions	Density Interactions	Voting Interactions
	(1)	(2)	(3)
Aid Treatment*Log Distance from Hometown*Aid Project Count	0.077		
	(0.092)		
Aid Treatment*Incumbent Percent*Aid Project Count			0.110
			(0.084)
Aid Treatment*Pop Density at School*Aid Project Count		0.174	
		(0.124)	
Aid Treatment*Aid Project Count	-0.412^{***}	-0.232^{**}	-0.206^{*}
	(0.131)	(0.115)	(0.114)
Aid Treatment*Log Distance from Hometown	-0.120		
	(0.098)		
Aid Treatment*Pop Density at School		0.147	
		(0.128)	
Aid Treatment*Incumbent Percent			0.098
			(0.091)
Log Distance from Hometown*Aid Project Count	-0.117		
	(0.072)		
Pop Density at School*Aid Project Count		-0.053	
		(0.086)	
Incumbent Percent*Aid Project Count			0.028
			(0.058)
Aid Treatment			
	(0.000)	(0.000)	(0.000)
Aid Project Count	0.181^{**}	0.147^{**}	0.127^{*}
	(0.094)	(0.080)	(0.080)
Incumbent Percent			0.126^{**}
			(0.063)
Log Distance from Hometown	-0.061		
	(0.074)		
Pop Density at School		-0.094	
		(0.090)	
Observations	2,612	3,375	3,482
Pseudo-R^2	0.008	0.002	0.007

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S16: Estimates from Main Text Figure 11 (Voting Interactions)

	Distance Interactions	Density Interactions
	(1)	(2)
Voting Treatment*Log Distance from Hometown*Incumbent Percent	−0.095	
	(0.094)	
Voting Treatment*Pop Density at School*Incumbent Percent		-0.268^{*}
		(0.161)
Voting Treatment*Incumbent Percent	0.117	0.003
	(0.109)	(0.096)
Voting Treatment*Log Distance from Hometown	0.175	
	(0.102)	
Voting Treatment*Pop Density at School		−0.173
		(0.129)
Log Distance from Hometown*Incumbent Percent	−0.019	
	(0.065)	
Pop Density at School*Incumbent Percent		0.032
		(0.121)
Voting Treatment		
	(0.000)	(0.000)
Incumbent Percent	0.117^{*}	0.175^{***}
	(0.079)	(0.070)
Log Distance from Hometown	-0.171^{**}	
	(0.073)	
Pop Density at School		0.010
		(0.090)
Observations	2,602	3,365
Pseudo-R^2	0.009	0.007

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

4 Additional Analysis

4.1 Power Analysis

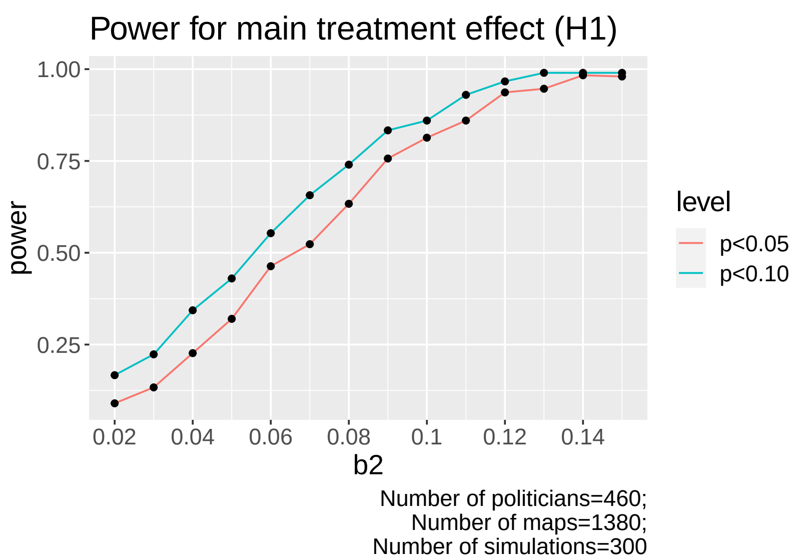
One possible reason why we cannot reject the null for some hypotheses is that our sample size is necessarily limited by the number of politicians in Malawi. This limitation on sample size makes it particularly hard to rule out the null for hypotheses that require multiple interactions (e.g., our transparency effect).

To aid in interpreting our treatment effects, in this section we show simulations of the statistical power of the study as designed. For each power simulation, we repopulate our dataset by sampling from the true distribution of schools and politicians. We then randomly assign treatment using simple randomization at the map and politician level.[4]

The results of this simulation are shown in Figures S1 and S2. For main effects (H1-H3), we obtain 80% power assuming a true normalized treatment effect (in log odds) of 0.09; which is equivalent to about a 50% increase on our expected baseline effect of z on y (0.06). While it is difficult to derive precise priors on treatment effects for a study like this one, we think these assumptions are reasonable given the low baseline levels of knowledge in our sample.

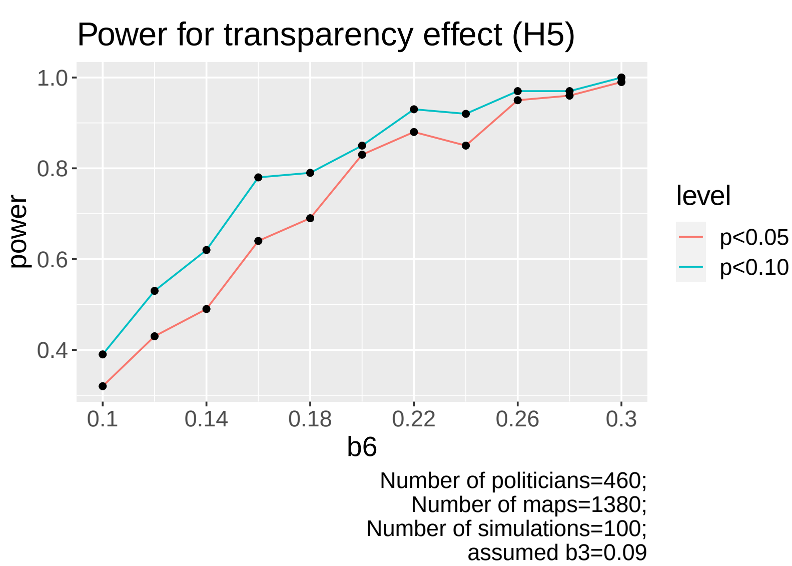
Our power to identify interactions is lower. We estimate that the power to identify significant interaction effects for the transparency arm (H4) is less than half that of our main treatment effects. We obtain 80% power assuming that the interaction is 2x the main treatment effect (at 0.18). Small treatment effects is a reasonable explanation for our inability to reject the null on H5.

Figure S1



Note: This figure shows the expected power of our study (y-axis) to rule out the null for H1 under different assumptions about the true treatment effect (x axis).

Figure S2



Note: This figure shows the expected power of our study (y-axis) to rule out the null for H5 under different assumptions about the true treatment effect (x axis).

[4] This is somewhat conservative since the transparency treatment was blocked on politician characteristics.

4.2 Multiple Comparisons Adjustments

In the main manuscript, we report uncorrected p-values for each of our hypotheses about the effects of information. It is possible that these over-state the overall evidence in favor of our hypotheses since they do not consider the multiplicity of hypotheses associated with each treatment arm. Here we show how our estimates differ after correcting for the false-discovery rate.

In our pre-analysis plan we proposed three families of hypotheses about the main effects of need information, foreign aid information, and political information. In our pre-analysis plan we also proposed additional hypothesis families which explore the ways in which the treatment might interact with different sub-groups. Since these are mostly intended to decompose the main treatment effects in order to evaluate mechanisms, these violate the assumptions of a standard false discovery rate correction and we do not include corrections for these families of hypotheses.

Following our pre-analysis plan, we adjust for the false discovery rate within each pre-registered family of hypotheses using the Benjamini-Hochberg correction; which generally has greater power relative to comparable methods (Benjamini and Hochberg, 1995). For comparison, we also show estimates using the more conservative Bonferroni adjustment. For consistency, we show estimates using two-tailed hypotheses for both directional and non-directional hypotheses.

To summarize the findings of this analysis, in Figure S3 we show how the p-values on our main hypotheses vary under alternative assumptions about multiple comparison, sample size and control variables. Below we clarify exactly how each multiple comparison test was executed and which hypotheses were included. We also show corrected p-values for all hypotheses within each family. As Figure S3 illustrates, the p-values on our treatment effects are larger after these corrections. However, particularly in specifications with controls, p-values on H1 and H2 remain near 0.10 (0.05 in a one-tailed test) after correction.

In Table S17 we show adjusted estimates for the need information treatment. In our pre-analysis plan, we proposed three main hypotheses of the effects of need information.[5] These hypotheses are listed in Table S17 as we originally formulated them in the pre-analysis plan. After adjusting for the multiplicity of hypotheses, we see stronger evidence in favor of a null hypothesis ($p = 0.15$ and $p = 0.1$). It is worth remembering however that our predictions for need information are directional, so these two-tailed tests may overstate the evidence in favor of a null.

In Table S18 we show adjusted estimates for the aid information treatment. In our pre-analysis plan, we only proposed one main hypothesis for the average effect of the aid information treatment (H1). However we also proposed that treatment effects might differ depending upon the frequency of donor interaction and the characteristics of the school (H2-H4).[6] Since H2-H4 are intended to decompose the main treatment effect, a standard multiple comparison correction is not appropriate or informative.[7] However, to remain as consistent as possible to our pre-specified approach, we nonetheless estimate corrected p-values. We show adjusted p-values both for the effects of treatment on the number of aid categories at a school (columns 2-4) and for the number of past aid projects (columns 5-7). The adjusted p-value estimates for H1 remain near conventional significance levels ($p = 0.12$ and $p = 0.09$).

In Table S19 we show adjusted estimates for the political support information treatment. In our pre-analysis plan, we proposed two main hypotheses of the effects of political information.[8] After adjusting for the multiplicity of hypotheses, the adjusted p-values for the main effects are above typical levels of statistical significance.

[5] These hypotheses are referred to as HB1-HB3 in the pre-analysis plan.

[6] These hypotheses are referred to as HD1-HD5 in the pre-analysis plan. Note that HD1 and HD3 refer to the same estimate with different hypothesized signs. Since we rely on two-tailed tests throughout, we can combine these two hypotheses in this table.

[7] Note that H2-H4 are not hypotheses about the treatment, but rather hypotheses about whether treatment effects differ across sub-groups.

[8] These hypotheses are referred to as HC1-HC2 in the pre-analysis plan.

Figure S3: Multiple Comparison Adjustments

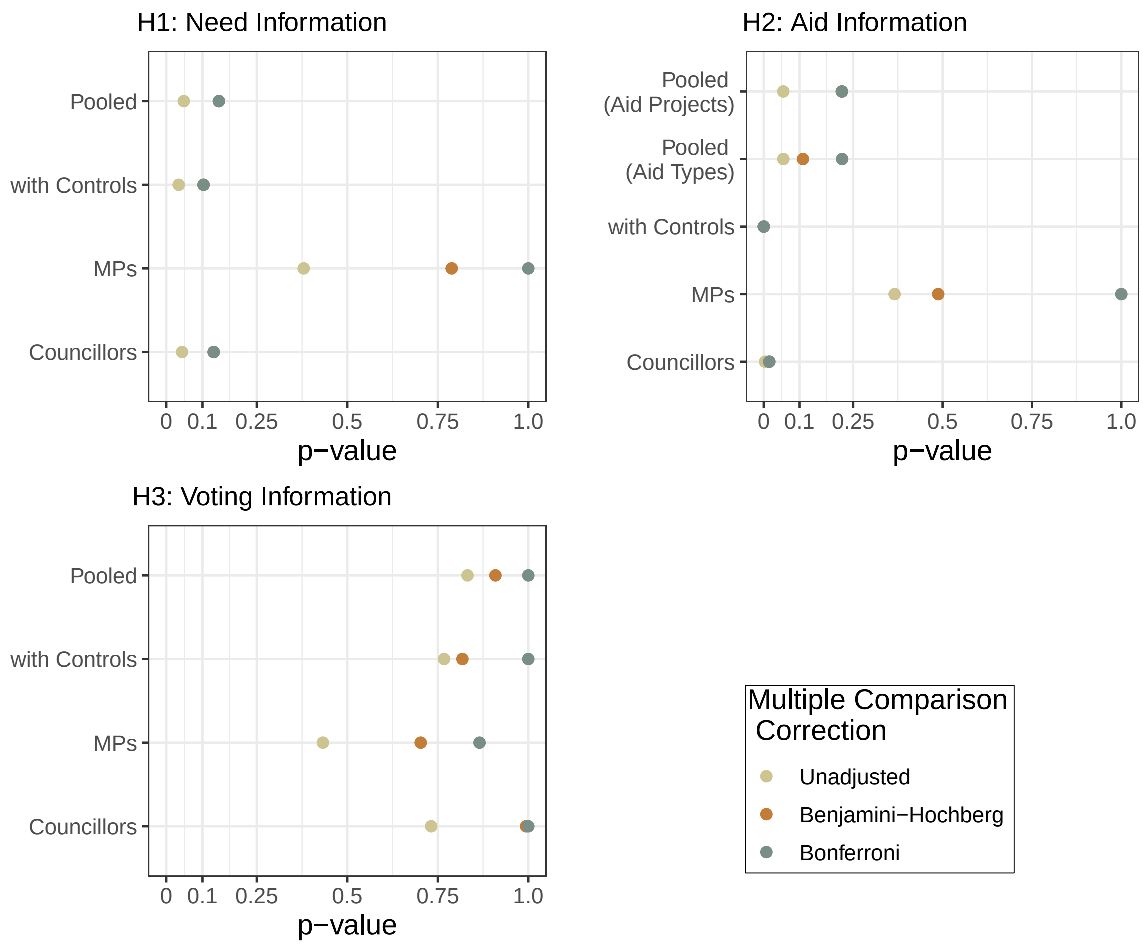


Table S17: Multiple Comparison Adjustment, School Need Information

Hypothesis	Unadjusted	BH	Bonferroni	Unadjusted with controls	BH with controls	Bonferroni with controls
H1. Politicians will be more likely to allocate to schools in areas with high need.	0.0484	0.1453	0.1453	0.0343	0.1029	0.1029
H2. Politicians will be be more likely to allocate to schools located in areas with higher support in the last election.	0.2247	0.3371	0.6741	0.3138	0.4707	0.9415
H3. Politicians will be less likely to allocate to schools located in their home community or where family members attend.	0.5241	0.5241	1.0000	0.9614	0.9614	1.0000

Table S18: Multiple Comparison Adjustment, Foreign Aid Information

Hypothesis	Unadjusted Aid Types	BH Aid Types	Bonferroni Aid Types	Unadjusted Aid Projects	BH Aid Projects	Bonferroni Aid Projects
H1. Politicians will be more likely to allocate to schools that have already benefitted from more past aid projects and where donors have provided more categories of goods.	0.0548	0.1096	0.2191	0.0546	0.2184	0.2184
H2. Treatment effect will be greater when politicians interact frequently with donors.	0.7903	0.7903	1.0000	0.5043	0.5043	1.0000
H3. Treatment effect will be greater where the politician did not receive a high proportion of votes.	0.0295	0.1096	0.1178	0.1630	0.3260	0.6521
H4. Treatment effect will be greater where schools are less needy.	0.2661	0.3549	1.0000	0.4279	0.5043	1.0000

Table S19: Multiple Comparison Adjustment, Political Support Information

Hypothesis	Unadjusted	BH	Bonferroni	Unadjusted with Controls	BH with Controls	Bonferroni with Controls
H1. Politicians will be more likely to allocate to schools located in areas with higher support for the politicians in the last election.	0.8320	0.9092	1.0000	0.7674	0.8180	1.0000
H2. Politicians will be less likely to allocate to schools in areas with high need	0.9092	0.9092	1.0000	0.8180	0.8180	1.0000

4.3 Assessing Experimenter Demand and Social Desirability

As discussed in the main text, one might worry that politicians are responding to the information provided in this experiment because of social desirability. In particular, politicians may believe that donors in general or our research partner, Tearfund, in particular expects them to respond to the information in a certain way. While we cannot completely rule out this possibility, one way to explore such effects is to see if responses to the treatment vary when politicians interact more with donors, or with Tearfund.

We conduct this analysis in Tables S20, S21, and S22. Overall we see little evidence of heterogeneous treatment effects. Politicians who have worked with Tearfund or worked more frequently with other donors are not significantly more likely to respond to the information treatments.

Table S20: Treatment Effects Conditional on Donor Interaction and Tearfund Knowledge

	All Surveys	All Surveys	All Surveys
	(1)	(2)	(3)
Need Treatment* School Need Index* Frequency of Donor Interaction	−0.015		
	(0.037)		
Need Treatment* School Need Index* Heard of Tearfund		0.056	
		(0.077)	
Need Treatment* School Need Index* Worked with Tearfund			0.072
			(0.103)
Need Treatment* School Need Index	0.090*	0.041	0.063
	(0.051)	(0.059)	(0.041)
School Need Index* Frequency of Donor Interaction	0.001		
	(0.025)		
School Need Index* Heard of Tearfund		−0.017	
		(0.055)	
School Need Index* Worked with Tearfund			−0.014
			(0.069)
School Need Index	0.033	0.046	0.039
	(0.036)	(0.042)	(0.030)
Observations	3,486	3,486	3,492
Pseudo-R^2	0.005	0.006	0.006

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S21: Treatment Effects Conditional on Donor Interaction and Tearfund Knowledge

	All Surveys	All Surveys	All Surveys
	(1)	(2)	(3)
Aid Treatment* Aid Project Count* Frequency of Donor Interaction	0.067		
	(0.107)		
Aid Treatment* Aid Project Count* Heard of Tearfund		−0.195	
		(0.228)	
Aid Treatment* Aid Project Count* Worked with Tearfund			−0.107
			(0.321)
Aid Treatment* Aid Project Count	−0.269*	−0.103	−0.183
	(0.156)	(0.173)	(0.122)
Aid Project Count* Frequency of Donor Interaction	−0.068		
	(0.074)		
Aid Project Count* Heard of Tearfund		−0.081	
		(0.161)	
Aid Project Count* Worked with Tearfund			−0.134
			(0.231)
Aid Project Count	0.185*	0.165	0.136*
	(0.112)	(0.123)	(0.085)
Observations	3,486	3,486	3,492
Pseudo-R^2	0.001	0.002	0.001

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S22: Treatment Effects Conditional on Donor Interaction and Tearfund Knowledge

	All Surveys	All Surveys	All Surveys
	(1)	(2)	(3)
Voting Treatment* Incumbent Percent* Frequency of Donor Interaction	−0.005		
	(0.089)		
Voting Treatment* Incumbent Percent* Heard of Tearfund		−0.248	
		(0.183)	
Voting Treatment* Incumbent Percent* Worked with Tearfund			−0.230
			(0.241)
Voting Treatment* Incumbent Percent	0.018	0.165	0.059
	(0.128)	(0.138)	(0.100)
Incumbent Percent* Frequency of Donor Interaction	−0.050		
	(0.064)		
Incumbent Percent* Heard of Tearfund		0.073	
		(0.131)	
Incumbent Percent* Worked with Tearfund			0.165
			(0.176)
Incumbent Percent	0.215**	0.121	0.134**
	(0.095)	(0.099)	(0.071)
Observations	3,476	3,476	3,482
Pseudo-R^2	0.005	0.005	0.005

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

4.4 Compliance and Validation

We took steps to validate that respondents correctly interpreted the treatment instruments, and we pre-specified several variables that we would use to test whether issues of compliance introduce bias into our estimates. First, we conducted a test of whether respondents could correctly interpret the maps we provided. Prior to participating in our experiment, respondents' were given an example map and asked to interpret the information provided. If they could not interpret the information, respondents were given detailed instructions to make sure they could correctly interpret the maps. Only 4% failed to understand the map on the first try. Of these, 76% were LCs, who tend to have lower levels of education than MPs. Second, we asked our RAs to record (1) whether respondents requested other schools than those shown on the maps, (2) whether respondents disputed whether particular schools were in their constituency, and (3) whether the respondent requested goods other than those Tearfund was provisioning.

In Table S23, S24 and S25 we show how our treatment effects differ across these measures. While there is some evidence of stronger treatment effects among those who understood the maps (especially in Table S24), we cannot reject the null of no difference between compliers and non-compliers.

Table S23: Treatment Effects by Compliance

	(1)	(2)	(3)
Aid Treatment* Aid Project Count* Misunderstood Maps (Q1.22)	−0.316		
	(0.339)		
Aid Treatment* Aid Project Count* Requested Other School (Q1.71)		−0.279	
		(0.733)	
Aid Treatment* Aid Project Count* Requested Other Goods (Q1.73)			0.002
			(0.758)
Aid Treatment* Aid Project Count	−0.181	−0.216*	−0.219*
	(0.126)	(0.118)	(0.118)
Aid Project Count* Misunderstood Maps (Q1.22)	0.059		
	(0.227)		
Aid Project Count* Requested Other School (Q1.71)		−0.019	
		(0.514)	
Aid Project Count* Requested Other Goods (Q1.73)			0.507
			(0.497)
Aid Project Count	−0.151	−0.148	−0.150
	(0.132)	(0.129)	(0.130)
Observations	3,492	3,492	3,492
Pseudo-R^2	0.021	0.020	0.021

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S24: Treatment Effects by Compliance

	(1)	(2)	(3)
Need Treatment* School Need Index* Misunderstood Maps (Q1.22)	−0.168		
	(0.121)		
Need Treatment* School Need Index* Requested Other School (Q1.71)		−0.006	
		(0.252)	
Need Treatment* School Need Index* Requested Other Goods (Q1.73)			−0.153
			(0.216)
Need Treatment* School Need Index	0.103**	0.082**	0.087**
	(0.042)	(0.039)	(0.040)
School Need Index* Misunderstood Maps (Q1.22)	0.241***		
	(0.088)		
School Need Index* Requested Other School (Q1.71)		−0.073	
		(0.162)	
School Need Index* Requested Other Goods (Q1.73)			0.126
			(0.155)
School Need Index	0.031	0.063*	0.057*
	(0.033)	(0.032)	(0.032)
Observations	3,492	3,492	3,492
Pseudo-R^2	0.022	0.020	0.020

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S25: Treatment Effects by Compliance

	(1)	(2)	(3)
Voting Treatment* Incumbent Percent* Misunderstood Maps (Q1.22)	−0.190		
	(0.304)		
Voting Treatment* Incumbent Percent* Requested Other School (Q1.71)		0.013	
		(0.519)	
Voting Treatment* Incumbent Percent* Requested Other Goods (Q1.73)			−0.483
			(0.551)
Voting Treatment* Incumbent Percent	0.044	0.032	0.041
	(0.097)	(0.093)	(0.093)
Incumbent Percent* Misunderstood Maps (Q1.22)	−0.048		
	(0.209)		
Incumbent Percent* Requested Other School (Q1.71)		−0.423	
		(0.402)	
Incumbent Percent* Requested Other Goods (Q1.73)			0.221
			(0.402)
Incumbent Percent	6.315	7.886	6.243
	(16.380)	(16.327)	(16.325)
Observations	3,482	3,482	3,482
Pseudo-R^2	0.020	0.021	0.020

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

5 References

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